carbon dioxide. These flames are therefore described by him as being "simply a hollow conical sheath of pretty uniform character." This is undoubtedly a true description; neither of these flames presents the appearance of double coned structure which is seen in such flames as cyanogen, carbon disulphide, ammonia, and others; and it is hardly possible that in a hydrogen or carbon monoxide flame there can be two distinct areas or cones in which different chemical processes are going on. It occurred to me that it might throw some light upon the real value of this cone-separating apparatus as an appliance for dissecting flames, to try its effect upon the single-coned flames of carbon monoxide and of hydrogen. When air was cautiously admitted into these gases, as they burned at the top of the tube, I found that the flame travelled quickly right down the tube, and did not stop at the narrower tube when the upward rate of movement was greater, and did not appear to leave any remnant at the top of the wider tube. I have no doubt but that Prof. Smithells has made this experiment, and with a similar result.

I have found, however, that by a slight modification of the apparatus, it is quite easy to drag down an inner flame from either the flame of carbon monoxide or of hydrogen. In order to do this, all that is necessary is to provide the top of the inner and narrower tube with a cap made of fine wire gauze, either copper or platinum. When this small addition to the original apparatus is made, and the experiment with carbon monoxide is repeated, it will be seen that as air is gradually introduced a portion of the flame descends the tube and sits quietly upon the wire gauze, and, in spite of the flame-extinguishing power of the carbon dioxide it there generates, a remnant of the original flame remains feebly burning at the top. In the case of hydrogen a similar result is obtained, a portion of the flame descending to the gauze, where it burns with a pale blueish flame, while the remnant burns freely at the top. These experiments show that whatever is the structure of the flame, a part of it can be torn away from the rest by the regulated introduction of air: that in order to divide a flame by this method it is *not* a necessary condition that the flame should consist of more than one "cone," or, in other words, that there should be two distinct areas of combustion. If, therefore, a "simple" flame like that of hydrogen, consisting of a single cone of uniform character, can be divided, the fact that other and more complex flames can also be so divided, does not seem to throw much light upon their structure. As soon as sufficient air has been admitted into a flame, of whatever burning gas, to produce a certain volume of an explosive mixture whose rate of explosion exceeds the rate of efflux of the gases, that exploding mixture will become detached from the remainder of the burning gas, and travel back down the tube. In the case of hydrogen, where a very wide margin exists within which mixtures of this gas and air are rapidly explosive, the admission of a very small quantity of air is sufficent to form such a mixture, and so drag down a comparatively small portion of the entire flame. In the space between these two flames there can only be water vapour as the product of combustion, atmospheric nitrogen, and the excess of hydrogen. The lower flame is a burning mixture of air and hydrogen in which an excess of air is taking part in the combustion, and represents a condition of things certainly not far removed from, if not identical with, the old phenomenon of air burning in hydrogen. It is difficult to see in what way the separation of other flames differs from this.

I have no doubt that everyone who has read the account of Prof. Smithell's lecture will have been struck, as Dr. Armstrong was, with the manner in which the classical researches of Dr. Frankland are brushed aside, and the difficult question as to the true causes of the luminosity of flame is settled by an appeal to the "opinion of the majority."

Without touching the question as to whether or not solid carbon is actually precipitated during the decompositions that are going on in a coal-gas flame, the recent experiments of Prof. Lewes leave no room for doubt that the first stage in the process of decomposition and condensation that goes on, is the production of acetylene, which is formed during the passage of the gas through the inner dark area of the flame, where no combustion is going on; that is to say, where the hydrocarbons are being simply strongly heated, but are not burning. This fact seems to have an interesting bearing upon some of the peculiarities exhibited by the well-known flame of air burning in an atmosphere of coal-gas. In this flame the air is in the inside, and the hydrocarbons upon the outside; it is in effect an ordinary coal-gas flame *turned inside out*. The formation of acetylene, instead of taking place within the flame, as in the usual conditions, in which case it has to pass through the heated area where it is further decomposed with probably the precipitation of carbon, is now produced upon the outer surface or periphery of the flame; it therefore largely escapes combustion or decomposition, and passes into the coal-gas atmosphere with which the flame is enveloped. Hence the flame is non-luminous, and hence also this constitutes the ready method for obtaining large quantities of acetylene first devised by Prof. McLeod. I am not aware that it has ever been noticed that during the combustion of this non-luminous flame there are produced, besides acetylene, other hydrocarbons of much greater density. That this is so is evident from the fact that when the flame has been allowed to continue burning for a length of time, the glass vessel in which it is contained becomes coated with a brown tarry film. This non-luminous flame of air burning in coal-gas can be rendered luminous by a simple device. If the vessel employed in which to burn it be an ordinary bulb-shaped paraffin lamp chimney, it will be seen that when the flame is in the middle and wide portion of the chimney it is non-luminous; if, however, it be thrust up into the narrow part, it at once shows signs of luminosity: the acetylene under these circumstances is reflected back into the flame, which, aided no doubt by the radiated heat from the glass, causes the luminosity. If the supply of air be regulated, the flame may be caused to curl over upon itself, whereby very beautiful vortices are obtained, in which Heumann's floating particles are well seen. There is an old experiment in which two flames of air in coal-gas are placed side by side, and so arranged that at will they can be caused just to impinge upon each other. At the point where they touch a small luminous area is seen to appear, the luminosity being probably due to the same causes.

G. S. NEWTH.

I AM unable to understand how Prof. Smithells can in any way suppose that I either have, or possibly could, cast any imputation on his honesty, "scientific" or otherwise; and I fail also to understand what has given rise to the impression, unless it be that the opening sentence of my letter—which I intended should convey a compliment—has been turned round and a meaning given to it which I never contemplated, and which it cannot fairly be made to bear.

I have always regarded NATURE as a journal which is willing to afford a fair field for the consideration of scientific problems, but the last place in which to raise, let alone discuss, personal questions. By publishing his lecture in NATURE, Prof. Smithells directly challenged criticism, and the only object and intention of my letter was to challenge the validity of certain of his arguments. That he should have taken the view he has, is to me a matter of deep regret. He has now stated his position very clearly, and the passage that he has been good enough to quote from my letter to Sir G. G. Stokes sufficiently defines mine. I fear that we must agree still to differ; evidently we look at these matters from very dissimilar standpoints.

HENRY E. ARMSTRONG.

The Postal Transmission of Natural History Specimens.

AT page 100, *ante*, you reproduce a circular letter, sent out by the Academy of Natural Sciences of Philadelphia, on this subject, the object of which is the very laudable one of establishing an international rate of postage for natural history specimens, based on that charged for *bona fide* trade patterns and samples. It is therein stated that the United States Post Office Department recently proposed to the countries comprised within the Postal Union a modification of the rates in favour of a charge so based, but that the Governments of very many of them declined to consider the proposal, and in the list there given Great Britain is included. No precise date for this refusal on the part of the British postal authorities is given, but presumably the date is not precisely recent. Early in 1891, several of our Natural History Societies agreed to approach the British postal authorities on this point, and a letter was addressed to the Secretary of the Post Office (the late Sir S. A. Blackwood) by Lord Walsingham, on March 18, 1891. A reply (which I have before me) to that letter, from Sir S. A. Blackwood, is dated April 13, 1891, and is published in the Proceedings of the Entomological Society of London, 1891, p. 14 (and probably elsewhere). An extract from the letter is to this effect:—"Your

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lordship will no doubt be glad to learn that so far as this Department is concerned, scientific specimens sent by sample post, and addressed to places abroad, will not be stopped in future; but I must state that this Department cannot guarantee the delivery of such specimens abroad, inasmuch as they do not come within the definition of sample packets as prescribed by the Postal Union." I may add that within the last month I have, on two occasions, sent specimens abroad by sample post with perfectly satisfactory results.

All naturalists will feel grateful to the Academy of Natural Sciences of Philadelphia for agitating in this matter. But it is to be regretted that the United States Postal Department should, in another way, continue to maintain a barrier against cheap transmission and interchange of specimens. The sample post can, in any case, only be used for small packets, but larger packages can now be sent to nearly all foreign countries by parcel post, the introduction of which was an inestimable boon. The United States Government stands almost alone in persistently refusing to co-operate in this respect. It is not for scientific men to inquire into what contracts that Government may have entered into with private carrying companies, or how far it may be influenced by hyper-protective susceptibilities; they can only regret the facts, and deplore the result.

Lewisham, December 8. R. McLACHLAN.

"Geology in Nubibus."—Mr. Deeley and Dr. Wallace.

MR. DEELEY will not have anything to say to ice conveying thrust *as a solid body*, which has been the sheet anchor of glacial geology for many a decade. He also repudiates Dr. Wallace's notion that regelation can in some way act as a compensating element when crushing supervenes in ice, and thus enable it under crushing pressure to convey thrust. So far so good.

Mr. Deeley, however, bids me turn to ice acting as a viscous body, a subject on which I have written a great deal in my recent book, which he does not seem to have seen.

There are two ways in which we can conceive a viscous body flowing on a flat plain: (1) by pure fluid, or what is commonly called hydrostatical pressure, in which the upper layers move up and down, and the lower layers alone have a horizontal motion; (2) by its particles rolling over each other. The former depends, of course, entirely upon the difference of level of two connected parts of the mass under consideration; the latter depends upon the slope of the upper surface of the fluid.

I contend, as Forbes contended, that in the case of a body so slightly fluid as ice, motion by hydrostatic pressure is practically impossible. The consistency and mutual support of the parts prevent the indefinite transmission of pressure in this way through ice, and nowhere have I seen or heard that in detached masses of a glacier cut off at either end by crevasses the ice rises in one place, and sinks in another, or that the walls of these ice rifts or the perpendicular ice walls in the arctic and antarctic regions or in scarped icebergs bulge out below in the slightest degree, as must happen if ice were to move in this method.

Forbes' experiments and measurements and patient examination of the problem proved that ice as a viscous body moves in fact by its layers rolling over each other, and that this motion is differential, being greatest at the surface and in the middle, and least at the base and sides of a glacier.

It is quite true that the rate of this motion on a flat plain would depend theoretically on the slope of the upper surface of the ice. It is established by experiment, however, that such motion is very largely confined to the surface layers, and when we approach the nether layers the motion quickly slackens, owing to the internal friction and drag of the ice particles. Even on inclined beds, glaciers have sometimes been found frozen to the ground. The evidence of a large number of observers is conclusive, that as glaciers reach the level ground, the motion, even of their upper layers, gradually stops. The masses of ice that collect on the flat Siberian Tundras do not move at all, nor do the thick horizontal ice beds examined by Dall in Alaska. Argument, experiment, and observation are therefore entirely against Mr. Deeley, upon whom the burden of proof rests. Perhaps he will explain what are the conditions under which he conceives his ice sheets to have been formed, to have been maintained, and to have moved. Mr. Wallace confesses that he does not like to face these mechanical issues, which are presupposed in all his reasoning. This is assuredly building on a quicksand, which is not a profitable experiment. He cannot be serious, either, in arguing that because I believe in Charpentier's view that the Alps were formerly higher, and consequently nursed bigger glaciers, I am therefore committed to Ramsay's extravagant notions, repudiated by nearly all explorers of glaciers, that the lakes of Geneva and Lucerne were dug out by ice. Charpentier's method, in such a case, would have prompted him to first prove the capacity of ice to do the work, and most people will agree that in a scientific argument this method is alone fruitful. H. H. HOWORTH.

30 Collingham Place, Earls Court.

The Viscous Motion of Ice.

IS not Sir H. Howorth wrong in assuming that there is no transmission of hydrostatic pressure in ice? Certainly Forbes was of opinion that such transmission existed, and was necessary to explain the remarkable parallelism between the motion of ice and of viscous fluids. It is a question of scale. Even a cup of treacle will not flatten out indefinitely; still less will a barrel of pitch; but I have no doubt a cubic mile of ice would flatten out, but to what extent is a question for calculation, not for dogmatic assertion. Unfortunately the first requisite of such calculations is wanting, as no determination of the coefficient of viscosity exists. Canon Moseley's experiments are clearly out of court, and in the interesting experiments of Mr. Coutts Trotter in 1883, the length of the portion of ice which took part in the shearing motion is not given.

May I add that the paragraph in Sir H. Howorth's letter of November 23, in answer to Mr. LaTouche, is distinctly erroneous so far as our limited evidence goes.

If Sir H. Howorth will draw to scale the observations of Prof. Tyndall at the Tacul on the side of the Mer de Glace, or those of Prof. Forbes, given on page 554 of his own book, he will see that while the velocity of the ice is greatest at the surface, the viscous yielding or differential motion is greatest at the bottom; and the curve into which a vertical line in the ice is thrown by the motion, is always convex towards the direction of motion, is relatively flat above, and strongly curved towards the base. This is exactly what we should expect on the viscous hypothesis, and justifies the application of hydrodynamical treatment to the problem, if only the necessary data were to hand.

19 The Boltons, S.W. JOHN TENNANT.
December 12.

Chemistry in Space.

IT may be of interest to your readers to know that the idea of the arrangement of atoms in space, which is looked upon as quite a modern one, is clearly put forth by Wollaston in his paper entitled "On Super-Acid and Sub-Acid Salts" (Phil. Trans. vol. xcviii. 1808, pp. 96-102).

He discusses the constitution of the two oxalates of potash; and I make the following extracts, but must refer your readers to the original paper for the full context. . . . "when our views are sufficiently extended, to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension. . . . when the number of one set of particles (combined with one particle), exceeds in the proportion of four to one, then, on the contrary, a stable equilibrium may again take place, if the four particles are situated at the angles of the four equilateral triangles composing a regular tetrahedron. . . . It is perhaps too much to hope, that the geometrical arrangement of primary particles will ever be perfectly known." Thus Wollaston's conception of the combination of four particles with another is exactly the same as our modern idea of the arrangement of four monovalent atoms (or groups) in combination with a carbon atom. The same idea is also developed somewhat later by Ampère in his "Letter to Berthollet" (*Annales de Chimie*, 90, p. 43-86, 1814), in which he considers the molecules as forming various geometrical figures dependent on the number of atoms contained therein.

JOHN CANNELL CAIN.

The Owens College, Manchester, December 14.

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